clusion that waves incident on the earth's surface would suffer reflection accompanied by splitting up of the simple condensational or distortional waves into two sets, one of each kind, so that, at distant stations, the arrival of the direct waves would be complicated by the arrival of reflected waves, which had travelled one part of their course as condensational, and the other as distortional, waves. The critical point at which confusion from this cause would arise is at about 120° distant from the origin, and the disappearance of the second phase, as a recognisable feature in seismograms at greater distances, is attributed to this cause; the records, which I had accepted as possibly indicating a diminished and retarded appearance of this phase, being interpreted as the arrival of the reflected distortional waves.

With regard to this explanation, I may say that the reality of the reflected waves, though accepted by many seismologists, and practically universally by the Germans and the whole school dominated by the influence of their work, still seems to me far from being established. The theory is based on the assumption of a globe of uniform constitution bounded by a reflecting surface, but this does not exist in nature, for the outer crust of the earth is composed of material which was long ago shown, I believe first by Prof. Rudzki in 1899, to be composed of material which cannot transmit simple condensational and distortional waves, but transforms them into more complex forms of wave motion. Nor have we reason to suppose that the lower surface of this outer crust presents a definite surface of contact between two media of different character, from which reflection could take place; rather it is to be expected that the transition is gradual and that the simple forms of wave motion, which can be propagated through the central portion of the earth, would be gradually converted into more complex forms, and become extinguished, in the surface layers. On the observational side, too, the case is not conclusive, for though the presence of reflected waves in the record has been claimed, more particularly in the case of earthquakes originating in the Malay Archipelago and beyond, their presence does not seem to be constant, nor by any means so conclusively established as the reality and distinctness of the first- and second-phase waves.

Accepting, however, the reality of reflected waves and the interpretation, offered by Prof. Wiechert, of the records accepted by me, with considerable hesitation, as possibly representing the arrival of the second-phase waves, it does not afford a sufficient explanation of the absence of the record of the arrival of the second-phase waves, travelling along the direct course from the origin, in seismograms from stations at and beyond 12,000 km. from the origin. This phase is well represented, and usually conspicuous, especially in the records of horizontal pendula with a moderate rate of travel of the recording surface, and up to the limiting distance, at which it disappears, forms a feature in the seismogram which should be recognisable even if superimposed on the record of reflected waves; for, apart from the hypothesis of a central core of material less capable of transmitting these waves, there is no reason for anticipating a diminution in the amplitude of the record at greater distances, but rather the reverse.

The length of wave path of the waves emerging at the antipodes of the origin is certainly greater, about 12,750 km., as against about 9500 km. for waves emerging at 10,000 km. from the origin, but, on the other hand, two wave paths starting directly downwards with a divergence of 1° will issue on the surface at a distance of about 222 km. apart, and two wave paths starting at an inclination of about 70° to 74° downwards from the horizontal and a divergence of 1° will reach the surface at some 10,000 to 12,000 km. from the origin, and at a distance of about 500 km. apart. Setting these two against each other, we have, on one side, the increased energy due to a more than twofold concentration of wave paths, and, on the other, the greater absorption due to about 30 per cent. greater length of wave path, the former of which should more than counterbalance the latter, so that the record of the direct waves ought to be more conspicuous at greater distances than between 10,000 and 12,000 km., up to which it is easily recognisable. I have examined most of the records obtained at greater distances previous to 1906, and some of later date, but have failed to discover the second phase, and it seems reasonable to suppose that this may be explained by the wave paths to these greater distances having encountered a different form of matter which is less capable of transmitting the second-phase waves from that traversed by the wave paths which do not descend so deeply into the interior of the earth.

Though this letter has run to a considerable length, I hope you will allow me space to refer to another passage in Dr. Walker's book, on p. x. of the introduction, where he refers to a paper by me (published in *Phil Trans.*, 1900) as the first application of the well-known theory of longitudinal and transversal waves to Milne seismograms. Had Dr. Walker verified his reference he would have found that the paper has nothing to do with Milne seismograms, and that it was the first published demonstration of the *threefold* character of the wave motion recorded at a distance from the origin, and incidentally an explanation of the failure of earlier attempts to interpret the records in terms of the two forms only, of longitudinal and transversal waves.

R. D. OLDHAM.

The Evidence for Spontaneous Generation.

IN reference to the letter of Profs. Farmer and Blackman in NATURE of February 12, it seems needful to state that only two of my tubes were opened in their presence. One of them showed, as I had predicted, bodies very closely resembling Torulæ, in large numbers. They were not, however, typical Torulæ, such as are represented in Figs. 1, 3, and 5, of my communication published in NATURE of January 22, and I am prepared to admit some doubt as to their nature. The other tube showed, as others of the same series had done, peculiar spores, which when shown together with their mycelium (as in Fig. 2) to an eminent fungologist, were said by him to belong to a mould allied to the genus Oospora. He had no doubt as to its nature; and I am certain that these moulds must have grown within the tubes after their sterilisation, in one case to the extent of producing, after sixteen months, two tufts plainly visible to the naked eye.

I am glad to learn that one of the colleagues of Profs. Farmer and Blackman is repeating my experiments, and trust he will, after a time, be able to solve their doubts.

H. CHARLTON BASTIAN.

The Athenæum, February 13.

The Wearing of Birds' Plumage—A Woman's Protest.

IT is very gratifying to find how earnestly the best papers are now taking up the cause of the various beautiful birds hitherto so cruelly and callously slaughtered for the sake of their plumage. The dealers in feathers seem to think that because they have embarked in that particular trade it must never

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be abolished, no matter if the most exquisite birds become extinct.

It is known that many trades have suffered severely from the advent of the motor-car. Whip-makers have scarcely anything to do. Harness-makers have also suffered, yet these trades could scarcely demand that motors should not be used because such might suffer thereby. And as the world becomes more thoughtful and humane, surely if birds are to be safe the plumassiers must go to the wall, and no great harm. There are other callings in which they must by degrees embark.

It is very strange that men do not more definitely show how very much they dislike seeing ospreys and humming-birds in women's hair or headgear. Men who are most feeling and know all about it, and keenly detest the cruelty that these ornaments involve, will sit by women at dinners and operas and not show in the slightest degree what they feel about these barbarous ornaments. After all, women only adorn themselves to please men, and if these had the courage to show how intensely they disliked, and were distressed, by these things, they would decidedly not be worn. To their intimates they could say, "How much more charming you would look with anything on your head or hat than that."

Of course, there is no denying the fact that woman is the sinner, and it seems very sad and shocking that all the trouble and misery brought upon birds with beautiful plumage is owing to the ignorance or cruelty of woman—*cherchez la femme*. Yes, alas woman—and woman alone—is the sinner. She will not listen to the voice of her sisters who *do* know, and who so gladly would, and could, put her in the right way of looking at the matter. As she adorns herself chiefly to please men, well, let them educate her, with scorn and strong words if her vanity or stupidity leave her cold to information kindly given.

There is no supply without demand. This holds good of every commodity; and let the demand once cease, and all the endeavours of the kind-hearted lovers of the beautiful to preserve birds now so ruthlessly destroyed for no purpose but the adornment of vain and stupid women will be needless.

There is such an abundance of lovely ornaments to be had. Natural or artificial flowers, exquisite ribbons, laces, &c., and if there must be feathers, then take some which require no cruelty to procure, and which the deft fingers of most clever workers can dye and trim into things of weird beauty, almost as pretty as the real thing, for glint and twist can be added to ducks' and fowls' feathers enough to satisfy a savage. These would not only save the birds, but their feathers, being no longer required, would come into ever-increasing demand, and give work to thousands of women who are always complaining that there is nothing much left for them to do. This makes so many of them force themselves into positions which males could occupy. Every woman who takes a position a man could fill prevents one man marrying. This is an aspect of the case seldom considered by women, and would be well for them to ponder on. One is glad of *any* argument to induce women to think and to act in such a way that the horrible cruelties associated with their feathered heads may in time be a thing of the past. There is no doubt if they *knew* the shocking cruelties perpetrated to obtain such an unsuitable adornment to any kind-hearted woman's head, they would certainly not wish the *real* ospreys and humming-birds' feathers to be procured for them.

Of course, imitation feathers would be cheap—to some women an unpardonable fault. Well, when the adornment *must* be expensive, there are jewels and laces.

O. L.

Specific Heats and the Periodic Law—An Analogy from Sound.

I AM much interested in Dr. H. Lewkowitsch's letter on specific heats and the periodic law, which appeared in NATURE of February 12. His suggestion, based on Guldberg and Wage's "mass law," of a reconciliation between Sir James Dewar's recent low-temperature experiments and Dulong and Petit's earlier experiments on specific heats, seems to me most valuable.

I am well aware that analogies are apt to be dangerous, especially when pushed very far. Nevertheless, I am proposing to put forward the analogy from acoustics which may interest some of your readers.

The experiments on which my analogy depends are performed on an ordinary pianoforte, and as they may be repeated by anyone, I will state the directions thus:—Very gently strike a high note (say C in alt) with "loud" pedal down and the finger soon removed; change to soft pedal and notice how long the note is audible as you sit at the piano. Repeat in all particulars with a lower note (say C, two or three octaves below). It will be found that the lower note persists very much longer than does the higher note. Next repeat everything in the same way, but strike powerfully instead of gently. Notice the time during which each loud note remains loud (or audible to a friend in the next room). It will be found that there is very little difference in the duration of the two loud notes.

I think the analogy to be deduced is fairly obvious, but I will state it nevertheless.

Very soft notes arise from wires when vibrating with small amplitudes; these wires correspond to atoms at very low temperatures, for atoms under such conditions vibrate also with small amplitudes.

On the piano a definite amount of damping (produced by pedal action) curtails the amplitudes of the compared vibrating wires in a ratio which approximately is *inversely proportional* to their respective masses—*i.e.* equal damping (equal resistance to motion) has the *smaller* effect on the *more* massive wire. The results of Sir James Dewar's experiments at low temperatures are echoed *pianissimo* by these vibrating wires.

Louder notes correspond to higher temperatures; the amplitudes both of wires and of atoms are wider. In these circumstances of higher excitement, it is found on the piano that about the same amount of energy is wanted to reduce equally the loudness of light and heavy wires, while in the calorimeter it was shown by Dulong and Petit that about the same amount of energy is degraded in reducing equally the temperatures of light and heavy atoms.

REGINALD G. DURRANT.

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X-Rays and Metallic Crystals.

IN NATURE (August 14, 1913), and later in the *Philosophical Magazine* (October, 1913), Keene gave an account of some interesting experiments on the transmission of X-rays through rolled metal sheets. In connection with his investigation it may be of interest to record some results we have obtained in recent work on metallic crystals.

Some preliminary experiments were carried out with annealed specimens. A lump of copper, for instance, was cut in two, and one of the pieces heated up to a high temperature and then allowed to cool gradually, whilst the other piece was left untreated. Beams of X-rays were allowed to fall at almost grazing incidence on the two newly cut surfaces, and the reflected

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